BEFORE THE STATE OF WASHINGTON

ENERGY FACILITY SITE EVALUATION COUNCIL

In the Matter of Application No. 2009-01: WHISTLING RIDGE ENERGY LLC; WHISTLING RIDGE ENERGY PROJECT	EXHIBIT NO. 6.04r

APPLICANT'S PREFILED REBUTTAL TESTIMONY

WITNESS #6: GREG JOHNSON

Q Please describe the purpose of this rebuttal testimony.

A I am testifying in response to the pre-filed direct testimony of Dr. K. Shawn Smallwood and Mr. Don McIvor.

Q Are you able to answer questions under cross examination regarding your testimony?

A Yes.

Q As an initial question, after reviewing the Smallwood and McIvor pre-filed testimony, do you continue to hold your initial opinion, based on all the avian use data collected

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for the WREP, that construction and operation of the Project will not result in a higher risk to birds than wind projects in other wind resource areas (WRAs)?

A Yes, my opinion remains unchanged. The surveys that were performed on the site are all consistent with the WDFW's guidelines (WDFW 2009). The protocols applied to the surveys themselves are widely accepted within this scientific field. The methods I used to assess the data in order to predict avian impacts are consistent with the approach I have taken on at least 76 wind projects across the U.S. and Canada, including ten projects in the state of Washington since 2003 when the WDFW guidelines were initially adopted. The conclusions regarding relationships between birds and wind turbines are consistent with what I have observed across habitats and locations. The data represents the best available science for predicting avian impacts at the Project site. The mitigation measures that are recommended implement Washington policy as expressed in the WDFW wind power guidelines and as those policies are applied by Washington EFSEC on three wind projects it has sited and oversees.

Q Let's begin first with the Smallwood testimony. He criticizes what he construes as the evidence used to support the SCA estimate of wind turbine impacts on avian populations:

> The Application appears to have relied on several types of empirical evidence to predict wind turbine-caused impacts at the proposed 75 MW Whistling Ridge wind energy project. These lines of evidence included a model based on fatality rates regressed on utilization rates, comparisons of exposure index values among species seen at the site, and a comparison of raptor nest density to nesting densities at other wind project sites. However, these approaches have led to inaccurate predictions of project impacts at other locations, and therefore should be examined carefully before relying on them yet again.

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(Smallwood Pre-filed Direct Testimony at 2:15-19)

What is your response to Smallwood's understanding of the empirical data that supported the ASC?

A This is a common theme that he returns to throughout the testimony.

Let there be no confusion on this point. Fatality rates in the ASC for raptors were based strictly on the estimated raptor use (abundance) at the site. This is clearly stated in the baseline avian surveys that support the application. Basing fatality rate estimates strictly on estimated raptor use (abundance) is consistent with the approach taken by WEST on all wind projects, including those with Washington EFSEC, and is the appropriate scientific approach to the analysis of predicting avian impacts with wind turbines.

There is other information contained in the baseline avian surveys, including exposure index information and raptor nest data. It is there for good reason, as discussed below, but the exposure index and raptor nest data were not used in the baseline report to predict fatality rates.

The exposure index and raptor nest density is additional information that establishes and defines the context of a project. It is my professional opinion that understanding project area bird populations and behaviors, as reflected by the exposure index, is critical to understanding the environment within which a mortality estimate is made. Likewise, an awareness of raptor nest density within the context of the area proposed for a wind project is information that may be useful to setting the context of the project.

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However, neither of those data sets is used discretely to determine mortality rates, which is appropriately based on estimated raptor use within the context of species abundance at a given site.

Q Your work on the WREP is predictive in nature, correct?

A Yes. When a wind project is proposed, there is no way of knowing exact numbers of bird fatalities that are likely to result from a project's ultimate development. Our task is to apply standard protocols and methodologies in order to gather the baseline data. With that data in hand, it is analyzed in accordance with standard methodologies. Based on the baseline data gathered, avian use estimated from the abundance of birds yields a mortality prediction.

The gathering of the data and its analysis is not done randomly, and it is not done easily. Project sites are diverse – geography, topography and biodiversity at each project site is different, and each presents its own unique challenges. However, siting agencies need to be able to rely on the quality and consistency of scientists' work in order to allow the agency to understand the likely effects of a project. This is one of the reasons why agencies such as the WDFW establish guidelines and protocols for conducting an impacts analysis. The work WEST performed for the WREP site utilizes the same WDFW protocols used to analyze other challenging sites in the state such as the Wild Horse project. This is a very large project with rare and fractured critical habitat, abundant raptor populations, federally endangered species and 149 turbines. WEST's work involves assessing the baseline conditions, making impact predictions, and conducting on-going monitoring work to support adaptive management through the project's operations. Despite its many biological challenges, application of standard protocols and methodologies has yielded a manageable, functional project.

The WREP Project is no different. It is situated in commercially managed forest habitat where on-going timber harvest occurs, heretofore unstudied for wind energy development, and sits in a region where contention has raged over the listing of the Northern spotted owl. These challenges are unique to the WREP site, just as every site presents its own unique characteristics. Every jurisdictional siting and regulatory agency faces the challenge of reviewing each project with its own unique characteristics. In this state, WDFW developed a framework of guidelines, now in its second iteration, to address this by suggesting baseline information appropriate for conducting an impact analysis to habitat, birds, and other wildlife as a result of wind energy development in any specific area.

The guidelines themselves are only advisory, as the WDFW does not presently have regulatory authority over wind power development within the state. However, the agency itself has significant expertise on the subjects of birds, other wildlife and habitats in Washington. The guidelines were developed with the input not just from governmental agencies with subject-matter knowledge including the U.S. Fish and Wildlife Service, the Washington Department of Natural Resources and Klickitat County (home to a considerable number of wind energy projects), but also with the participation of non-governmental wildlife special interest groups including Audubon Washington, Seattle Audubon and The Nature Conservancy. The culmination of the work of these and other stakeholders has generated a robust set of guidelines that WEST and its clients apply in order to develop the body of information that will be

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reviewed by the WDFW when commenting on proposals for wind energy projects and evaluated by EFSEC or local regulatory agencies when evaluating a project for siting.

I have personally used these guidelines on ten projects in the state of Washington, including the WREP, in order to make predictions regarding impacts from wind development. EFSEC has sited, conditioned and oversees three wind projects, all of which were evaluated under the WDFW guidelines.

This does not mean that all scientists conducting baseline studies will generate identical predictions. The methodology for predictive modeling involves some exercise of discretion in selecting relevant factors gauged to particular sites and species, making assumptions in the absence of certain data, and involves subjective decision-making based on the scientists' training and experience doing baseline studies in accordance with the jurisdiction's protocols.

It does not follow, however, that "prediction failures are caused by fundamental shortfalls in the assumptions and methodology used to make the predictions" as Smallwood states at 3:4-5. All predictive modeling involves judgment calls based on a host of factors in order to make a prediction. It is scientifically and factually incorrect to state that any prediction that is not borne out by the actual event is the result of a fundamental shortfall in an assumption or methodology. A variety of factors can and does influence the actual outcome of a predicted event.
This is anticipated by the state of Washington. Both the WDFW wind power guidelines and prior siting conditions of the EFSEC embrace the concept of adaptive management to ensure that the predictions that were modeled are, in fact, monitored

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over time and evaluated in order to adaptively manage the situation in response to the facts as they are borne out on a project.

Q Smallwood has generated a table (Smallwood Pre-Filed Testimony at 3:7-14) of wind energy projects for which raptor fatalities per MW/year was predicted by WEST or others, followed by his own prediction, and the results of WEST's predictions then compared to reported fatality estimates following development and operation of those facilities. What are your thoughts regarding the table Smallwood has developed and the apparent purposes for which he offers it?

A First, it is unclear what criteria Smallwood used in deciding which Washington and Oregon wind projects to include in this table. Many of the fatality estimates in the table were made years ago, when there was little available fatality data (and certainly less than there is today) to inform predictions. For example, the baseline study for the Oregon Klondike project was conducted in 2001 and early 2002. No quantitative estimate was made for raptor fatalities at Klondike; the baseline report stated that they would be "nonexistent to low" based on the raptor use data. As predicted, raptor fatalities at Klondike I and III were actually 0. On the other hand, Smallwood misrepresents the raptor fatality estimate of 0.11 for Klondike II as being 11 times higher than predicted. This conclusion is impossible to draw: the original prediction was *not* quantitative, i.e., zero mortalities at Klondike II. Instead, on this early project, the prediction was for a *nonexistent to low* mortality rate. Smallwood simply decided that the meaning of "low" meant zero quantitatively, and then makes the leap to a worst-case scenario in order to entice the reader to conclude WEST's work is unacceptably flawed. (This, by the way, demonstrates that this predictive modeling is not an absolute science: it involves the making of assumptions. All scientists must do

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it.) The particular interpretation by Smallwood, choosing to quantify as 0 the “zero to law” qualitative statement, dramatizes an outcome that does not correctly present the prediction made by WEST. Table 1 does make one point: predictive modeling is just that: predictive.

Fortunately, in Washington predictive mortality estimates do not exist in a vacuum. TACs review, study, and monitor projects and at times, operations are modified based on the TAC’s assessment of the data over time as compared to pre-construction predicted estimates.

The methodology used by WEST on the WREP has been used by many agencies in a variety of jurisdictions to inform agencies in reviewing and approving projects. In the many years I have been doing initial predictive modeling of wind energy and avian impacts, I have not had permitting agencies find my methodologies and formulas for predicting fatality rates disallowed. This includes the ten wind projects I have worked on in the state of Washington where, as I indicated above, I routinely apply the WDFW siting guidelines.

Q Over the years, have you modified the methods and assumptions you use to generate avian impact predictions?

A Yes. For example, in the early years of wind development, we had insufficient data to generate regression analyses, which is a guide to assist in providing a prediction and range of mortality. This analysis, as a tool, was unavailable to us in the raptor mortality predictions made on Klondike.

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Q Is this the regression analysis that Smallwood criticizes in his pre-filed testimony at page 3:20—23 through 5:1-18? For example, at 5:13-15, he states

> The relationship purported by Johnson is represented in a graph that WEST, Inc. has repeatedly used, but which has not been peer-reviewed or published in the scientific literature.

This suggests that peer-review or publication in scientific literature is the only means by which a methodology or tool in conducting impact assessment can be deemed acceptable in the wind industry. What is your response?

A Yes, Smallwood criticizes the use of the regression analysis graphing that WEST utilizes as a tool in its avian impact assessment. I understand that Smallwood frequently authors academic papers, based on his research, that are published in scientific periodicals. This is one means of putting forth one's views and theories, and Smallwood is prolific at it. It is not the only way one becomes proficient at what one does. WEST also puts forth and shows its work, but in a different venue – the permitting arena. This regression analysis graph has probably been used in well over 50 reports that WEST has generated for wind project proposals. Such graphs have been reviewed by numerous state wildlife and natural resource agencies, local and state permitting agencies, and USFWS without any significant push back about which I am aware.

Q Please explain your understanding of Smallwood's critique of use of this tool, and explain why it is used by WEST.

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A Initially, I must clarify two things. Part of his criticism is focused on the narrative explanation in the DEIS for the WREP. *See, e.g., Smallwood Pre-Filed Testimony* at 3:22-23 through 4:1-2:

"*Mean overall bird use in the study area was low compared to these other wind resource areas studied: ranking 19th compared to 24 other wind resource areas…*" and, "*Mean annual raptor use was 0.28 raptors per plot per 20-minute survey, which is a standardized way to measure use in order to compare results to avian use at other sites*."

The author of the DEIS relied upon and interpreted work by WEST, but WEST was not its author. This discussion centers on raptor fatality predictions using bird use values, not actual mortality estimates once fatality surveys have been conducted. I am assisting the author in responding to comments to the DEIS (such as Smallwood's criticism here), in order to clarify WEST's formulas for estimating mortalities.

Also, Smallwood's considerable focus on WEST's regression analysis is not commensurate with the use of the graph in WEST's work. The regression analysis is only one of several diagnostic tools in the approach taken by WEST when doing predictive modeling of raptor mortality at wind projects. Again, I reiterate that our predicted avian mortality rates are based on estimated raptor use within the context of species abundance at a given site, as it has been well documented that raptor mortality is related to the abundance of raptors, which is intuitive.

Part of Smallwood's criticism of the use of the regression analysis graph, found in his pre-filed testimony at 4:17-21, is as follows:

> The regression relationship in *Figure 8* of DEIS App. C-4 exemplifies psuedoreplication in correlation analysis, which is a fundamental experimental design flaw that is routinely warned against in statistics textbooks. The regression is based on two clusters of data, one from wind projects located mostly in the Pacific Northwest and the other from two projects located nearby each other in California. If the variation in the graph was more representative of the two regions—Washington/Oregon versus Central California—than of the individual project sites, then the sampling units were really the regions and not the project sites

Psuedoreplication is treating data points as being independent when in fact they are interdependent. In other words, he is saying that the values from the Pacific Northwest projects should not be treated as separate data points independent of each other because they are from the same region and might be interdependent. However, the graph he refers to is not just a region by region comparison. The graph includes every wind project for which there are preconstruction avian use estimates and post-construction fatality estimates. Although the values at the lower end of the regression are mostly from the Pacific Northwest, they also include values from Wyoming and Minnesota. It just happens that more data are available from the Pacific Northwest due to the large number of projects built and studied here. Smallwood continues, asserting at 4:22-23 through 5:1-5:

> In presenting their graph, Johnson and Erickson (2010) presented a value for the coefficient of determination, r^2, but they neglected to present an error term. Furthermore, they presented the relationship as significant, and the DEIS repeated that conclusion along with the prediction, based on the regression, that 0 raptors would be killed by Whistling Ridge wind turbines (page 3-79).
>
> The coefficient of determination is an index of both response and precision, but the reader must be familiar with regression analysis to visually assess the degrees to which variability or precision contributed to r^2. A more direct measure of precision is the root mean square error (RMSE) of the regression, otherwise known as standard error. In my experience, RMSE can serve as a diagnostic tool for deciding whether r^2 was influenced more by leveraging from outliers or from psuedoreplication.

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Smallwood offers his opinion as to which diagnostic tools and elements he prefers to use. However, both measures are valid and often both might be used.

The reader should understand what this regression analysis graph demonstrates. It shows that low raptor use generally means lower mortality. Likewise, high raptor use generally means higher mortality will be the pattern.

Now, If I take Smallwood's suggestion and drop one point in the clusters as he recommends ("Another diagnostic test is to omit data from one of the clusters to learn whether the regression slope would change significantly" – *Smallwood Pre-Filed Testimony* at 5:5-6), the pattern of lower raptor use generally equating to lower raptor mortality and higher raptor use generally equating to higher mortality is <u>still</u> the pattern. However, Mr. Smallwood would have the reader draw a different conclusion:

> In fact, omitting the two data points from Central California project sites converted a strongly positive slope to a negative slope (see dotted line in Figure 1), and the revised regression line was a better fit to the data, based on RMSE (RMSE = 0.0567, which was a third of the value for the pseudoreplicated regression slope). In cases like this, when two data points determine whether an estimated regression slope is strongly positive or negative, the analyst should not use the regression equation to make predictions. It was inappropriate to predict that 0 raptors would be killed by Whistling Ridge. *See, Smallwood pre-Filed Testimony at 5:6-10*.

In layman's terms, what he is saying here is that the negative slope indicates there will be more bird deaths as the bird use estimates get lower. This is simply absurd to think that the fewer raptors you have in the area, the more mortality you would expect.

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Insofar as the prediction in the DEIS of 0 raptors being killed at WREP, recall that the actual prediction interval in the baseline report prepared by WEST was stated to be 0 to 0.25 raptor fatalities/MW/year, an oversight that will be corrected in the final EIS.

Q So, the regression analysis graph, despite Smallwood's disaffection, is simply a tool that WEST has added to its methodology when assessing avian impacts at wind energy projects over the years. Are there other changes that you have made to your approach over time?

A Yes. As Smallwood points out in his pre-filed testimony at 5:15-17, the utilization surveys that contributed to the data in his Figure 1 (regression analysis graph) were often inconsistent with current survey protocols and scientific practices, and often lasted less than one full year. This is in contrast with the current recommendations of the WDFW Guidelines and U.S. Fish and Wildlife Service Wind Turbine Guidelines Advisory Committee's recommendations. As a result of the standardization of survey and data-gathering protocols, WEST in late 2007 standardized to a weighted average by number of days in each season for the overall raptor use value calculation. This should explain the differences between the Whistling Ridge regression analysis and those contained in previous reports.

I note that Smallwood, at pages 16-17, suggests that the utilization data might also have been flawed based on non-consecutive seasons studied at some projects. Wind siting guidelines such as those of the WDFW suggest that surveys cover all four seasons. Nowhere does WDFW state that these have to be consecutive seasons. It should be made very clear that there is no suggestion inference that the seasons of study at WREP were selected with the purpose of choosing those with low utilization

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rates. This is something that is wholly impossible to predict going to into any given season. Surveys are started, stopped, interrupted, and completed based on a variety of factors that range from market conditions including low power prices, high load growth projections, tight equity markets, changes in ownership, legislative mandates on renewable energy, or moratoria that can defer completion of surveys until the underlying issues are resolved.

Nonetheless, Smallwood suggests at page 5:19-20 that inter-annual variation in both fatality rates and utilization rates at a given project site can easily exceed the range of variation depicted between project sites in the regression analysis graph included in his pre-filed testimony. The inverse could also certainly be true. This misses the point, in any event, and is confusing to the reader. The purpose of the graph is to show that higher raptor mortality is expected when you have higher raptor use, nothing more, nothing less. Smallwood's comments on inter-annual variability do not make any sense with regard to this graph or its purpose. While it is not clear what his point here is regarding avian usage and avian mortality, his suggestion of higher use/mortality rates and the inverse low usage/low mortality rates at Diablo Wind confirms what the graph is intending to show, e.g., when the lowest raptor utilization occurred at Diablo Wind, the raptor mortality was also the lowest.

Q Smallwood does not object just to the surveys supporting the regression analysis graph generally, but also suggests that WEST conducted inadequate surveys on the WREP project specifically. Given your level of experience not just with wind projects in general, but considering the ten projects in Washington state alone that you have worked on, do you have any reaction to his criticisms?

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A First, the adjudicative record developed in this matter should reflect that WDFW has confirmed that it is satisfied, without qualification, that the surveys performed by WEST on the WREP site are consistent with those guidelines.

As for his suggestion at page 5:17-18 that WEST made no effort to measure habitat and topography at the WREP site, these items were assessed on site and presented as Figures 3 and 4 in WEST's baseline report.

The baseline study itself, including all 261 surveys on a very small wind energy site, has been carefully reviewed by WDFW. The agency has no objections to the baseline surveys. WEST used standard protocols in performing all the surveys suggested by the WDFW guidelines. WEST did not participate in developing the guidelines. It simply applies them in the manner that it understands to be consistent with and desired by the WDFW. To do otherwise, despite Smallwood's urging, would be in gross derogation of the good faith with which wind developers in Washington have opted to comply with WDFW's advisory-only siting guidelines.

To make my point more clear, the Altamont Pass, where Smallwood has performed the vast majority of his career wind research - is one project area that is distinct from WREP just as the hundreds of other wind project sites across the county are distinct from WREP. One project does not and should not dictate all future protocols or methodologies broadly used within the industry. Absent legislative or agency directive from the state of Washington, WEST should not, will not, and cannot employ protocols and survey methodologies inconsistent with those recommended by the WDFW. These guidelines provide written, clearly understood, science-based, predictable, objective standards that have been adopted for many reasons, most

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importantly to ensure consistency within wildlife agencies in how agency biologists and policy-makers evaluate projects, and to address concerns raised by various stakeholders, including environmental organizations.

This includes the way WEST conducted diurnal avian utilization surveys done at WREP, with which Smallwood takes issue. To date there have been no large incidents of migrating songbird fatalities at wind farms, unlike what has been experienced with communication towers. Radar studies have shown that songbirds migrate across broad fronts and are not typically concentrated along any features. Wind turbines are very different from communication towers, which have historically had high songbird mortality. Wind turbines, unlike communication towers, do not have guy wires, which are blamed for most of the songbird mortality associated with such towers. Additionally, the lighting regime at a wind energy facility is totally different from communication towers. Furthermore, wind turbine heights are generally below the heights at which most songbirds migrate. Taken as a whole, for all of these reasons The Wildlife Society, in a landmark publication on wind energy and wildlife, concluded that fatalities of passerines from wind turbine strikes generally are not significant at the population level (Arnett et al. 2007).

Our survey methods for visible airspace observation, used on over 50 wind energy projects across the U.S., have never generated any concern or objection by any regulatory or permitting agency, including the WDFW, which has reviewed many wind energy avian surveys over the years. This is a non-issue within the entire wind industry with the lone exception being Smallwood. To be clear, the 800-m radius used during surveys of WREP refers to horizontal distance from the observer, not height above the observer. The protocol used to collect avian use data at WREP is

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consistent with the protocol used across the country. Efforts are made to place survey stations where visibility out to 800 m is either unhindered or encompasses most of the plot circle. In the case of the WREP site, it is so small that on occasion, the surveyed areas overlapped each other.

Because all studies are conducted in the same manner, it is appropriate to compare avian use rates among studies. The purpose of the survey is not to count the absolute number of birds. Instead, it is designed to obtain an index to bird use that can be used to assess risk at the site compared to other facilities where similar data have been collected using an 800-m plot. Therefore, correcting for visible airspace is not necessary or required in these studies contrary to Smallwood's suggestion. I know of no regulatory or permitting agency that requires corrections to visible airspace.

Nocturnal species in the project vicinity include owls. WEST did not conduct nocturnal visual surveys for owls in its baseline surveys. The owl surveys were conducted at the WREP site by Jeff Reams of Turnstone Environmental. Mr. Reams will be available as a witness in the adjudicative hearings.

Owls are identified by their call, an audible survey method rather than a nocturnal visual survey. Based on my review of Reams' surveys, no Northern spotted owls and very few barred owls were observed at the site. The available data do not indicate that owls are a significant concern at this site. The USFWS has vast experience on the Northern spotted owl in Washington given its ESA listing status. Significantly, the USFWS has reviewed the surveys and data on the WREP site, and it has expressed no reservations regarding Northern spotted owl impacts or other owl species at this site.

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Q Based on his review of other scientists' baseline survey and mortality predictions work on wind projects across the western United States, Smallwood declares at pages 15 through 17 that avian mortality is likely to be 2.44 times higher than that predicted by those other scientists. If WEST and other scientists are all using both state and federal agencies' guidelines for assessing avian impacts in order to site wind energy projects, how is Smallwood alone coming up with such disparate predictions?

A The fatality rate estimated by Smallwood for WREP was 33 raptors/year, or 0.44 raptors/MW/year. Raptor fatality rates at 13 facilities in the Pacific Northwest have ranged from 0 to 0.29 and averaged 0.09/MW/year (*Smallwood Pre-Filed Testimony* Table 1, uncorrected for inclusion of Buena Vista, a project that is not in Washington/Oregon). The raptor use data collected at WREP, done in consultation with the WDFW and in accordance with its protocols and guidelines, does not suggest raptor mortality would be higher at WREP than other projects with similar raptor use estimates, and certainly not as high as what Smallwood predicts.

The distinction lies in the way that data about avian mortalities is gathered. As is seen above, there is a broad range of data and human assumptions that go into these calculations. For example, it is virtually impossible to observe, gather, and analyze all avian carcasses in the vicinity of wind turbines in order to correlate the injury/death to wind turbine strike or other causes. Turbine sites are routinely searched, but it is inevitable that scavengers will account for some elimination of carcasses before they can be observed. Even for those carcasses that remain, the efficiency of the searchers' efforts can affect results. Human factors, such as always assuming that wind turbine strike is the cause of injury/death in the absence of other information, will also affect predictive mortality rates. As a result, methods known as

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estimators are applied to address how to handle, in calculations, the possibility that some carcasses may not be found. Basically, these estimators take observed fatality rates and divide them by an estimate of probability of a carcass being available for detection.

Smallwood states that bird and bat fatality rates are underestimated due to a bias in the estimator used by WEST. We use an estimator known as the Shoenfeld estimator (Shoenfeld 2004), which demonstrates the estimator it uses generally to be unbiased. The Bighorn project, one of the projects Smallwood included in his Table 1, was studied by a different consultant , Northwest Wildlife Consultants. NWC used a different estimator, known as the Huso estimator (Huso 2010). Huso (2010) has demonstrated that her estimator is generally unbiased. She also has shown that her estimator and the Shoenfeld estimator give similar results when the search intervals are large (e.g. 14 – 28 days), which is the case for most of the studies in the Pacific Northwest. Consequently, both the Shoenfeld and Huso estimators are recognized within the scientific field to be unbiased estimators for projects such as the WREP.

Smallwood, in his pre-filed testimony at 15:9, indicates that he uses an estimator known as the Horvitz Thompson estimator (Horvitz and Thompson 1952). The Horvitz Thompson estimator does not allow carcasses to be detected more than once. In other words, if a searcher misses a carcass on a search, but it is there to be found on the next search, the Horvitz Thompson estimator doesn't allow for that. Basically, once a carcass is missed once, the estimator assumes it can never be found again. That assumption injects a considerable problem with the use of both Huso and the Horvitz Thompson estimator, because it has been demonstrated in studies that fatalities that are missed the first time have a good chance of being picked up in

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subsequent searches (Arnett et al. 2009). Not accounting for this probability of finding carcasses during multiple searches leads to an overestimate of fatality rates in both Smallwood's estimator and the Huso estimator.

The National Wind Coordinating Collaborative is nearing completion of a methods and metrics guidance document for studying wind/wildlife interactions (Strickland et al. in press). This guidance document delves extensively into the use of various estimators for estimating bird and bat fatalities at wind energy facilities. According to this document, more research into the robustness and properties of these estimators for use in fatality studies is needed. Different estimators should be used depending on whether the average carcass removal time is longer or shorter than the average search interval. When removal time is less than the search interval, the guidance document recommends that the Shoenfeld (2004) or Huso estimator (Huso 2010) could be used. When the removal time is greater than the search interval, the Shoenfeld estimator may underestimate and the Huso estimator may overestimate fatalities depending on the assumptions related to searcher efficiency over time.

There is no current estimator that is unbiased in all situations. When removal time is less than the search interval and with moderate levels for searcher efficiency (30% - 70%), the guidance document recommended that the Shoenfeld or Huso estimator be used.

Despite the fact that there is no current estimator that is unbiased in all situations, WEST has been consistent in its use of a single widely accepted estimator, Shoenfeld, on all its work in the Pacific Northwest, including baseline studies and mortality predictions it has made on all three Washington EFSEC wind siting projects. In

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addition, the estimators used by WEST for several projects in the Pacific Northwest have been published in peer reviewed scientific journals (Johnson et al. 2002, Johnson et al. 2003, Johnson et al. 2004) and were not questioned by any of the reviewers. While it is unlikely that there will ever be consensus on which estimator is best in all situations, differences in fatality estimates between the two estimators are relatively minor when search intervals are wide (e.g. 14-28 days), which is generally the case in the Pacific Northwest. Upon issuance of the NWCC final report, it will be reviewed by WEST and likely others to determine if its conclusions merit a different approach in the future on any specific project.

Smallwood et al. (2010) recently published a study comparing his 'novel' approach to conducting carcass removal trials with what he terms the 'conventional' approach (consistently used by WEST)[1]. The purpose was to evaluate the effects of the different methods on estimated avian fatality at a particular wind power facility in California. Table 5 in that paper summarizes the results. However, it also reveals what I believe to be a serious flaw in the paper. The 95% confidence intervals (mean±1.96*SE) of all of the Smallwood et al. (2010) estimates and all but one of the 'conventional' estimates contain 0, yielding a nonsensical result indicating that estimates using both his and WEST's methods have confidence intervals that overlap. In laymen's terms, the results indicate no significant differences between the two methods. A second major flaw in the study was that Smallwood et al. only did carcass removal trials using their "novel" approach and did not compare their results concurrently with other approaches. It is simply not sound science that they compared carcass removal rates from national average data to the estimates they got

[1] The word "novel" is Mr. Smallwood's own choice of words, not mine, admitting the novelty of his own unique approach.

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from their one study and then, to take their scavenging results from a different study area and adjust fatality estimates for a select few sites in the Pacific Northwest. I reiterate here that WEST has consistently used the conventional estimator when conducting its mortality estimates in Washington.

I do not agree that the results on the WREP site, generated using our consistent and conventional approach seen by WDFW and EFSEC on other Washington projects, should be discarded here in favor of a "novel" approach that is demonstrated to be flawed by the author's own study.

I make this lengthy point for a particular reason. It is troubling that Smallwood has selected some, but not all Pacific Northwest wind projects for his Table 1 (and gratuitously includes a California project evidently for the point that he helped co-author the study along with WEST, apparently trying to give added credibility to its predictions) in order to independently determine that he "had to replace" their results with some that he generated using national averages:

> Underestimates of fatality rates in the Pacific Northwest might be partly caused by reliance on mean days to carcass removal as an adjustment for scavenger removal rates (Smallwood 2007), but some of the scavenger removal trials were sufficiently flawed that I had to replace their results with national averages in Smallwood (2007). Under-estimated fatality rates have been used to predict fatality rates of planned projects, which may be one reason why predicted fatality rates have so often been wrong (Table 1).

Smallwood has to replace nothing. The methods used by all of the Table 1 studies used statistically adequate sample sizes for carcass removal trials. It doesn't make any sense to throw all that data out and instead use "average" data from throughout the nation instead of data collected on site, which will always provide a better

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representation of local scavenging rates regardless of how it is collected. There is no call to “replace” the viable data in order to manipulate averages into a higher mortality prediction.

This approach of discarding data or substituting his own data, sometimes inexplicably other than to substantiate his position, is repeated at page 19, Figure 10. Smallwood here generates his own regression analysis. He uses WEST’s data and then adds in his own data from other wind resource areas without disclosing the sources or selection criteria for the additional data. WEST’s regression analysis is limited to modern wind turbines. Given that the vast bulk of Mr. Smallwood’s actual on-the-ground observational research experience is at the Altamont Pass Wind Resource Area, it is possible that this Figure 10 included some projects from California. This would be problematic because the APWRA had thousands of obsolete, older model wind turbines. Starkly different wind turbine models, such as the old turbines at the original Altamont Pass prior to its current repowering, are not appropriate for comparison to modern wind energy facilities. The old versus new models have significant differences including turbine sizes, support structures, and rpms. Modern monopole turbines with no guy wires have greatly minimized the incidences of avian mortality. In short, Smallwood’s use (or misuse) of inappropriate comparables yields an unreliable result.

Without disclosing the additional data that Smallwood injected to generated the Figure 10 regression analysis, it is impossible to draw any conclusions whatsoever about the different results generated.

Q Are there other areas of Smallwood pre-filed testimony that you can elaborate on?

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A Yes. At page 20:15-17, Smallwood notes some skepticism that the DEIS accurately reflects a result generated by WEST from a hypothesis test that would support the use of the exposure index as a predictive tool. We agree that the DEIS insufficiently portrays the intended use of the exposure index. The purpose of this model is to provide some insight into which species observed on the site might be the most likely to collide with turbines. It is not meant to accurately predict which species will occur as fatalities and was definitely not used to predict the level of bird fatalities. In the baseline avian survey report it was acknowledged that the index considers relative probability of exposure based on abundance, proportion of daily activity spent flying, and proportion of flight height of each species within the zone of risk for turbines likely to be used at the wind energy facility. The exposure index analysis is based on observations of birds during the daylight period and does not take into consideration flight behavior (e.g., during foraging or courtship) or abundance of nocturnal migrants. It also does not take into consideration habitat selection, the ability to detect and avoid turbines, and other factors that may vary among species and influence likelihood for turbine collision. For these reasons, the actual risk for some species may be lower or higher than indicated by this index. This clarifying information will be added in the final version of the EIS.

Nevertheless, Smallwood's conclusion at 20:21-22 that exposure indices "appear[] to be completely ineffective as a predictor of fatality rates caused by wind projects" is in complete derogation of WEST's work product and the facts borne out in the Pacific Northwest. As discussed in my opening rebuttal comments and restated here for clarity, the exposure index is not, nor does WEST hold it out to be, a predictor of mortality. Its usefulness lies in informing the scientist of contextual data that may be relevant to the actual mortality prediction. For example, the exposure index for many of the Pacific Northwest sites predicted that red-tailed hawks and American kestrels have higher risk than less common raptors. In fact, these species are the most common fatalities. Similarly, horned larks often have the highest exposure index of all passerines. In fact, they are the most common passerine fatality observed at most of the sites.

In a similar vein, Smallwood at 21:2-12, deliberately misrepresents the analysis in the DEIS on raptor nests as that of WEST's work in the baseline analysis, when in fact the two documents are obviously disparate in their conclusions. WEST's baseline avian study report did not attempt to use raptor nest density to predict raptor mortality. Raptor mortality was predicted based on actual raptor use of the site only. This is clear and obvious from a reading of the baseline avian study report.

Q One fact that you and Smallwood agree on is that there is no operating wind farm in a forested habitat in the Pacific Northwest that could generate monitoring data that would inform our understanding of wind energy operating in a Northwest forest. Beyond that fact, Smallwood at pages 23-15-23 through 24:1-7 suggests that statements in the ASC and DEIS reveal a lack of understanding in the habitat concept, and are inappropriate in an informational document. Please respond.

A First, it cannot be disputed with a straight face that the WREP site is a forest habitat being actively managed for on-going commercial timber harvest. The WEST baseline study is not intended to place a lower social value on the type of habitat that is present. The project site is simply not an intact, old-growth unmanaged forest. It is generally acknowledged that even-aged, managed forests provide far less suitable

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habitat for most avian species than uneven aged, natural forests (e.g., Buchanan 2005). Even aged, managed forests are sometimes referred to as "green deserts" due to their lack of wildlife abundance and diversity. As such, construction of a wind energy facility at WREP would have a much lower potential for wildlife impacts than construction of a wind energy facility within natural forests. To suggest that wind energy development is not compatible with managed forests, which provide only degraded habitat for wildlife, suggests that wind energy development would not be appropriate for any forested landscape in the western U.S., natural or managed.

Instead, the Whistling Ridge project provides an optimum location to obtain data on wildlife impacts that might be used to inform decisions and impact predictions for wind energy facilities proposed for other managed as well as unmanaged, natural forests. The WDFW and DNR are both in receipt of the baseline avian studies prepared for this project. Neither agency is suggesting that because it has never been done before, wind energy should not be allowed at this site. To the contrary, WDFW concurs with WEST's methods and protocols used on the WREP site. Likewise, neither agency has suggested that several generations of every possible species in the area be allowed to pass in order to determine whether they will ever return to the site before any wind energy facility is allowed. Such an approach to wind development is scientifically inconsistent with the WDFW guidelines and is unrealistic.

In fact, WDFW's Guiding Principle No. 3 of its wind power guidelines urges consideration of degraded habitat for wind projects. Ignoring the nature of the site, which undeniably is an industrially impacted degraded forest, and substituting it instead with data akin to an unmanaged, intact non-timber harvested forest would destroy the scientific integrity of the baseline and devalue the work entirely.

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Q At page 25:3-8, Smallwood offers his review of a study that suggests bats are attracted to wind turbines, chasing the blades as they forage for insects and in some instances experience barotraumas, meaning lung hemorrhaging due to a loss in pressure at the blade tips. What has your research revealed about the relationship between bats and wind turbines?

A First, we now know that turbines are not a strong natural attractant to bats. A night vision study recently completed in Illinois, the results of which were presented at the recent (October 19-21, 2010) National Wind Coordinating Collaborative meeting in Lakewood, Colorado (Johnson et al. 2010), found that 82% of bats flying past turbines did not show any reaction to the turbine, and only 2.1% inspected a turbine. Because 82.0% of bats did not show any reaction to turbines, these data imply that turbines are not a strong attractant to bats at the scale examined.

As for the barotraumas hypotheses, a study is currently being conducted at Illinois State University to examine barotraumas using bats killed at wind turbines and those killed by hitting buildings, where barotraumas would not be expected. The following is an abstract from a paper submitted to summarize this project (Rollins et al. 2011):

"Using bats salvaged from turbines at a wind energy facility in central Illinois, and salvaged from buildings in Chicago, IL, we tested the two leading hypotheses regarding bat fatalities at wind turbines: barotrauma vs. collision. 83% percent of building collision bats showed no signs of broken bones (similar frequency as collision killed birds) showing that a lack of broken bones does not necessarily exclude collision as a cause of death. 90% of all bats found at the wind energy facility showed some physical trauma consistent with death by collision. 20% of wind farm bats that were in good enough condition to examine for eardrum rupture had ruptured eardrums indicative of possible auditory barotrauma; however, 14% of those also showed physical trauma that could potentially cause a rupture. Therefore 6% is a conservative estimate for potential barotrauma cases in bats examined for auditory

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barotrauma. Therefore, the hypothesis that barotrauma is the dominant cause of mortality cannot be supported in bats found at this wind energy facility."

The facility addressed in the study is a modern wind energy facility with 1.65-MW turbines that has experienced moderate bat fatality. This, of course, is not a completed study and the excerpt above reflects preliminary conclusions only. Further study is certainly appropriate on the subject.

Of the 14 species of bat likely to occur in the WREP study area (Johnson et al. 2009), only four – hoary bat, silver-haired bat, little brown bat, big brown bat– have ever experienced known fatalities at wind-energy facilities, indicating the other species have much lower susceptibility. Acoustic bat surveys at the WREP were able to classify bat calls to frequency groups that roughly correspond to groups of relative risk. Most of the bat activity recorded at ground level involved high frequency bats. The high-frequency bat species are not typically associated with turbine fatalities. Based on data from 10 wind energy facilities in the Pacific Northwest, hoary bats and silver-haired bats have comprised the majority (93.4%) of fatalities. These species are considered low frequency species and they were more commonly recorded on Anabats elevated on met towers. At the WREP site, activity levels for low frequency bats were not high compared to activity levels at sites with high bat mortality. Implementation of the on-going monitoring requirements of the WDFW's Wind Power Guidelines at Washington wind energy projects will continue to generate additional baseline data about bats and wind turbines. At the present time, there are no results from Washington wind farms, nor from the bat surveys conducted at the WREP site, that suggest unexpected or unacceptably high mortality rates.

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Q The bulk of the discussion to date on wind energy facilities has focused on predicted mortality rates. At page 25 of his pre-filed testimony, Smallwood introduces the concept of displacement that may result from turbine-avoidance behavior in certain species. Can you discuss what is known to date regarding turbine avoidance behaviors?

A Several studies have found that there is minor displacement of some, but not all bird species, and there is no consistency, among wind energy facilities, about the avoidance levels of a single species from which to draw definitive conclusions. When avoidance has been documented, it has usually been limited to 200 m or less. To date, no studies of avoidance behavior have been conducted at wind energy facilities in forested landscapes; all have been done in open grassland and shrublands. While there are no studies to date that assess avoidance behavior in forest settings, it is possible that the visual obstruction from the trees, as well as familiarity with tall vertical structures, may influence avian avoidance behavior differently than for species that live in vertically unobstructed environments.

Q At page 26:7, Smallwood states that there is no scientific basis for the DEIS conclusion that the level of avian and bat mortality at the WREP is not anticipated to be sufficient to negatively affect the population viability of any single species. Do you agree with this contention?

A No, I do not agree. To date, only one study has been conducted to examine this specific issue and it did not show a population level decline. Hunt (2002) conducted a 4-year radio telemetry study of golden eagles at the APWRA and found that the resident golden eagle population appeared to be self-sustaining despite high levels of

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fatalities. The effect of these fatalities on eagle populations that were wintering within and adjacent to the APWRA was unknown. All 58 territories occupied by golden eagle pairs in the APWRA in 2000 remained active in 2005 (Hunt and Hunt 2006).

The Wildlife Society prepared a landmark publication on wind energy and wildlife and concluded that fatalities of passerines from turbine strikes generally are not significant at the population level (Arnett et al. 2007). We believe that the reason no other studies have been conducted to determine if wind projects have caused any population declines of any bird species is that measured fatality levels have been low enough that no agencies have expressed concern over population level impacts and required such studies.

The topic of population impact necessarily invites a discussion on what is known about avian populations in the first instance. In his testimony at page 26:21-23 through 27:1-9, Smallwood is very critical of WEST's use of a population estimator based on breeding bird survey (BBS) results and provided by the Partners in Flight (PIF) North American Landbird Conservation Plan. Unsurprisingly, Smallwood suggests that WEST's use of such data is misplaced, and that a bias created by data having been gathered along roadways where certain raptors congregate and hunt renders the information unusable.

What Smallwood fails to discuss is that this report is not only the best available data on nationwide avian populations, it is the only such report. (Blancher et al. 2007). While perhaps surprising, the USFWS does not maintain data that would allow estimates of all avian populations in the country.

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PIF is a cooperative effort involving partnerships among federal, state and local government agencies, philanthropic foundations, professional organizations, conservation groups, industry, the academic community, and private individuals. The mission of PIF includes 1) helping species at risk, 2) keeping common birds common, and 3) voluntary partnerships for birds, habitats and people. PIF recognized the importance of generating estimates of bird populations across the U.S., which were lacking for most species and most regions. They used relative abundance counts from the North American Breeding Bird Survey (BBS) to form the basis of their bird population estimates.

Although PIF acknowledges that the BBS was not designed specifically to produce population estimates, and there are difficulties to overcome as a result, there are important advantages to having the information for review and use as appropriate. The data from across much of North America have been collected according to a single standardized method. Surveys employ random start points and directions, thus enhancing regional representation of the avifauna (roadside bias notwithstanding), and the data are readily available for the bulk of North American land birds. According to PIF, the population estimates are rough approximations for land birds breeding in the U.S. and Canada, and the results and the underlying data of this first massive effort to estimate population numbers for all North American land birds can be used for several different purposes (http://www.partnersinflight.org/).

In order to prepare cumulative impacts analysis, estimates of population sizes are required. Otherwise, it is impossible to determine how raptor fatalities associated with wind energy development could affect populations and therefore lead to cumulative impacts. The only population estimates available for most bird species in

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the Pacific Northwest are those estimates calculated by PIF. Although these estimates may not be completely accurate for all species, they are the only ones available and therefore represent the best available science for this use. Use of the best available science to make informed decisions is standard practice in ecology and wildlife biology, and is acknowledged as foundational in the WDFW wind guidelines. In the United States, many of the laws governing conservation and management stipulate that the best available science be used as the basis of policy and decision making. One such law, the Endangered Species Act, requires that decisions on listing a species as threatened or endangered be made on the basis of the "best scientific and commercial data available." Similarly, National Standard 2 of the Magnuson-Stevens Fishery Conservation and Management Act states that conservation and management measures shall be based on "the best scientific information available." In addition, the U.S. Environmental Protection Agency has emphasized the role of best available science in implementing the Clean Water Act (Sullivan et al. 2006). Therefore, use of the PIF population estimates, given that they represent the best available science, is clearly accepted practice and is in fact mandated by many U.S. laws for making informed policy decisions.

Finally, estimated raptor fatality rates in the cumulative impacts analysis for the WREP (Johnson and Erickson 2010) suggest that raptor mortality associated with 7,600 MW of wind energy comprises a small proportion of the population and further comprises a very small proportion of natural mortality. Therefore, actual bird population sizes in the analysis area would have to be substantially lower than estimated by PIF before conclusions regarding the lack of cumulative effects would not be considered valid.

Within the more narrow context of population size for golden eagles on the Columbia Plateau Ecoregion in Washington and Oregon, Smallwood intimates that the 2010 cumulative impacts analysis (Erickson and Johnson, 2010) that includes a golden eagle population estimate of 1,700 simply cannot be correct. He concludes, at 27:13-16, that for 1,700 golden eagles to occur on the Columbia Plateau within Washington and Oregon, the population density would have to be nearly as high as recorded in the Altamont Pass, or nearly one nesting pair per 19 km^2. Smallwood is simply incorrect on the math. A breeding population of 1,700 individuals implies a maximum of 850 eagle pairs. That portion of the Columbia Plateau Ecoregion in Oregon and Washington is 150,664 km2 in size. Therefore, the density of eagle pairs is one pair per 177 km2, not one pair per 19 km2, which is nearly a 90% lower density and a far more plausible situation.

The cumulative impact assessment done by WEST, using the best available cumulative population numbers known, uses the same methodology as when predicting the project-specific analysis in the baseline study analysis. It is unsurprising, then, that Smallwood's assessment of the cumulative impact numbers would also follow his own "novel" approach to assessing avian impacts, with commensurate higher predictive ratios than those of WEST.

One area where there is a dearth of cumulative impacts information is that of wind energy projects built in forested habitats. In the Pacific Northwest, there are only four wind projects that have seriously been proposed for development in forested habitat: the WREP; Coyote Crest in Lewis County, WA; Radar Ridge in Pacific County, WA; and Middle Mountain in Hood River County, OR. A cumulative impacts analysis such as that in the EIS must take into consideration those projects that are reasonably

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likely to be built. At the present time, the Middle Mountain project has been tabled, and the Radar Ridge project faces considerable hurdles for it to ever be built. In my professional opinion, I do not consider construction of either of these projects to be reasonably foreseeable.

I am in the process of reviewing the avian information on these projects in order to assist the DEIS author in finalizing a cumulative impacts analysis of known, forested habitat wind energy projects likely to be built in the Pacific Northwest. That analysis will utilize information drawn from a decade of experience with avian populations in the Pacific Northwest, including those relevant data sets from the recently updated 2010 Columbia Plateau Ecoregion cumulative impacts study prepared for Klickitat County. While not all of the information contained in that study is applicable to a forested habitat such as the WREP, the Columbia Plateau Ecoregion is immediately adjacent to the WREP site and the areas share some common data. Even with my preliminary review of the Coyote Crest and Radar Ridge raw data, I remain confident in the conclusions drawn regarding non-significant cumulative impacts of this project when combined with other similar forested habitat projects.

The WREP site is a forested site managed for more than a century for commercial forestry. It does not present a natural or native coniferous forest condition. The avian baseline surveys (including raptor surveys) did not simply rely on data from other projects—WEST surveyed and analyzed this particular project site in accordance with WDFW wind guideline protocols and obtained biological information specifically applicable to the WREP site. Those reports were provided to WDFW. The agency reviewed them, and has met with WEST to discuss them. WDFW itself finds that these studies meet the agency's guidelines. To date, the relationship between raptor

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use and mortality at wind projects has been fairly consistent across habitats and locations, and there is no reason to believe that the relationship between raptor use and mortality would be different at the WREP site just because the habitat is different. This is the identical scenario presented to EFSEC, WDFW and WEST at the Wild Horse project. It was the first Washington wind project sited in an area containing rare, fractured critical shrub-steppe habitat and presents unique avian presence – sage grouse - that generated considerable concerns. Protocols were implemented and surveys conducted with WDFW's close collaboration to ensure appropriate data generation. The operational project is carefully monitored, and the Wild Horse TAC has been thoroughly reviewing the monitoring data as a part of its adaptive management process. There is no reason to believe that the WREP cannot and will not be appropriately conditioned, operated and managed with monitoring oversight provided by EFSEC in response to its own unique features.

While no similar data exist for constructed wind energy projects in managed Pacific Northwest coniferous forest habitats that might inform impact predictions for this project, the data WEST has obtained and generated, especially when synthesized with predictive information on the other proposed forest habitat wind projects, will represent the best available science for predicting avian impacts at the WREP site.

Parenthetically, I note that Smallwood indicates that during his development of a tool for screening proposed wind energy sites for avian impacts in California, he discovered that "forested sites pose greater hazards to more bird species, including special-status species at wind resource areas." *See Smallwood Pre-Filed Testimony* at 30:3-5. The study to which he references is unpublished and unavailable for review. I cannot comment thereon, other than to note that it is typically state regulatory and

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permitting agencies who comment on and embrace the validity of such approaches in order to successfully navigate project review and approval. I have no knowledge of where the state of California stands on the validity of this new "tool". It is also not apparent from the reference whether the tool is specific to coniferous forests managed for commercial forestry, versus forests in a more pristine, native habitat condition.

Smallwood's reliance on select conclusions in the Klickitat County EIS regarding forested habitat hazards are of no avail on this point. While there are statements in the Klickitat County EIS about forested habitat, it is important to remember that these comparisons were for an area that is predominantly grassland and shrub steppe, with very little forest cover. In addition, many of the forested areas that are present in Klickitat County are small areas of white oak, which is considered a priority habitat by WDFW. In that situation, it makes some sense to avoid forested areas when there are extensive opportunities to develop wind projects in grassland and shrub steppe, which are common habitats. Coniferous forests such as those at Whistling Ridge are not considered a sensitive or priority habitat by WDFW. They are very extensive and are not considered a limited habitat by any means. It is inappropriate to take conclusions from Klickitat County regarding development in forested areas where forested areas are very rare, and apply them to an area that is entirely forested.

Q One last areas of questions focuses on conditions that may be imposed on the WREP. Do you agree with the mitigation measures Smallwood suggests EFSEC adopt and impose for the WREP project at pages 30 through 32?

A I do agree that some of his recommended mitigations are appropriate. Sampling is a universal and widely accepted practice of estimating any parameter in wildlife

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biology or other sciences. However, one does not need to census every turbine to get an accurate estimate of fatality rates. For example, USFWS's FACA (Federal Advisory Committee Act; Wind Guidelines Advisory Committee 2010) guidelines do not recommend searching every turbine unless the entire wind project consists of 10 turbines or less. An 'every-turbine' census condition for WREP, then, appears unnecessary and inappropriate. Neither EFSEC nor WDFW has guidelines on this point, and EFSEC's reference to the USFWS standard, which in this case counsels against such a condition, may be most appropriate.

Using two teams of searchers is completely unnecessary. Searcher efficiency is a matter appropriately addressed by training, not volume, and does nothing to address scavenging rates. It is Smallwood's opinion alone that the scavenging rate factors are fraught with biases. The conventional methods for doing these surveys are included in the FACA guidelines, which state that carcass removal and searcher efficiency trials should be conducted using accepted methods. The methods currently in use at Pacific Northwest wind projects apply methodologies that enjoy broad acceptance among the wind industry's diverse stakeholders with the exception of Smallwood and his 'novel approach'. His methodologies are not only not accepted broadly in Washington, they are not used at all.

Utilization surveys are not routinely done on every project for monitoring, and no explanation or objective is offered for why they are being suggested here. Utilization surveys could be an adaptive management tool that could look at displacement by comparing utilization surveys to those conducted prior to construction and see if there is a drop in the number of birds. However, there is no indication at the WREP project, based on the scant studies ever done on displacement theory, that suggests such

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surveys would be an appropriate condition for operation of the WREP. Additionally, the suggestion for weekly intervals is unfounded. Intervals vary based on study objectives, none of which are articulated here.

The formation of a TAC is already a recommendation of the WDFW guidelines, and is routinely required for wind projects. The project applicant's pre-filed testimony indicates that it agrees with the propriety of a TAC. However, telling EFSEC who should sit on a TAC is not well-taken, and I strongly counsel against it.

It may not be scientifically inappropriate to have the TAC suggest, at an early stage, adaptive management BMP's broadly applied within the industry and which may be appropriate depending on scientifically valid results of Project operations monitoring. I do not believe that a TAC should ever be deprived of considering adaptation as facts about a project emerge, however.

Never have I seen an agency, whether it be permitting in nature such as EFSEC or regulatory in nature such as WDFW, require a developer to post a performance bond in order to ensure permit compliance with wildlife criteria or to provide offset or compensatory mitigation for impacts to birds and bats. This is not supported by prior EFSEC decision-making, the WDFW guidelines, or the permitting framework of any jurisdiction I have ever worked in. There are no guidelines for the establishment of a bond, the terms of when it could be called, who the beneficiaries would be, or how the proceeds would be applied. It would be virtually impossible to determine the amount of the bond to "ensure compliance and offset or obtain compensatory mitigation for impacts". There are plenty of tools that a siting agency such as EFSEC has in order to enforce the terms of a project permit. Demanding money to guarantee

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wildlife condition compliance is not one of them. There is nothing in the WREP data, or any policy of the state of Washington or its representative agencies, that counsels otherwise.

I disagree with the conclusion that there is little that can be done to effectively mitigate bird and bat fatalities once a project is operational (although posting of a bond certainly does nothing to advance a solution). This conclusion is specifically incorrect in regard to bats. Several studies have found that bat mortality at wind energy facilities is highest during nights with low wind speeds. Studies conducted in Pennsylvania (Arnett et al. 2010), Alberta, Canada (Baerwald et al. 2009) and Germany (O. Behr, University of Erlangen, unpublished data) have all shown that raising turbine cut-in speeds so that turbines are not operable during low wind speed conditions can reduce bat fatality rates by an average of 50% to 77%. The TAC, early on, may suggest this as an adaptive management BMP specific to WREP if monitoring data demonstrates a level of bat mortality that scientifically calls for mitigation. Smallwood is also incorrect in assuming that forest cover will impede carcass searcher effectiveness, thereby reducing awareness of the need for mitigation. While I agree that there are challenges to a forested habitat, they are not insurmountable. WEST has done surveys at forested sites in the eastern part of the United States that presented the same concerns. There are things an operator can do to address this, including maintaining the area around the turbine to keep the vegetation down in search areas. Searcher efficiency trials are also available to determine how many birds/bats are being missed. The results of such trials inform how to adjust the search efforts. Thoughtful work in the implementation of carcass monitoring is appropriate at every wind project, not just in forested habitats.

Q Do you have any final thoughts about the challenges to the professional quality of your work in the baseline avian study and analysis done for the WREP?

A Briefly, yes. While over the past 23 years I have provided environmental and statistical consulting services and contract research nationally and internationally to government, industry, and private organizations, wildlife research with an emphasis on wind power development has been a specialty in my career. The wind power industry has matured during that time, and development of wind has occurred across a broad and diverse range of habitats, ecoregions and geographic areas. My own experience has grown commensurately, having worked on at least 76 wind energy development projects in 17 states in the United States and in Alberta, Canada. In that process, I have been largely responsible for gathering project area data using techniques developed over several decades of on-the-ground work and in conjunction with siting guidelines of myriad regulatory and permitting agencies, filtering the data through a broad body of experience on different projects, and making predictive estimates of impacts utilizing that breadth of knowledge, including application of what we have learned about wind and birds and bats over decades of wind energy project operations. This includes ten projects I have worked on specifically within the state of Washington since 2003. My colleagues at WEST, who apply the same methodologies and approach to wildlife impact assessments, have also worked on projects in Washington, including three wind projects sited by EFSEC. I believe that WEST's work, including my own, is reflective of widely-accepted, conventional industry and scientific standards, and not only is consistent with but implements the WDFW wind energy guidelines and is reflective of my understanding of EFSEC's general approach to siting wind energy facilities, which reflect the policy of the state of Washington.

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Finally, I emphasize that in his testimony, Smallwood repeatedly insists that EFSEC adopt his “novel” approaches, and that EFSEC depart from best available science, precedent, and wildlife agency guidelines that have been consistently applied by state and federal agencies. Smallwood acknowledges the “novelty” of his methodology and his perspective. His perspective does not reflect the biological experience and data yielded from wind energy development throughout the United States or specifically in the Pacific Northwest.

Q Let’s turn to the pre-field testimony of Mr. Don McIvor. At page 3:15-18, he indicates raptors generally rely on north-south trending mountain ranges to support their migration. Is this correct and if so, is it significant to the WREP?

A While raptors do rely on north-south rides for migration, the onsite data collected for the WREP project do not indicate high raptor use during spring or fall such that the presence of such north-south ridges in the area will present an issue for the WREP.. McIvor does not suggest that the onsite data is incorrect; he is merely noting a biological fact that exists independent of any effect at the WREP.

Q What is the significance of McIvor’s observation, at 3:19-21, that the Columbia River, which lies about two miles south of the site, appears to be an important migratory corridor for birds, and an important winter congregation area for wintering Bald Eagles?

A Again, McIvor is not suggesting this presents an issue undisclosed in the baseline studies. He is noting biological facts, not applying them to the WREP data in order to draw any inferences. There is no evidence indicating that bald eagles come up several miles from the Columbia River to the WREP, nor would that be consistent with Bald Eagle behavior. Despite the proximity and utility of the Columbia River for winter congregation, only 3 bald eagles were observed at the WREP over the course of a year, indicating very little use of the site by this species.

Q In addition to the same comment as Smallwood about the DEIS's misinterpretation of the utility of the exposure index as a predictive tool, McIvor expresses concern regarding the collision risk modeling as a predictor of mortality. Is the collision risk model intended to predict mortality?

A No. The purpose of the collision risk model is to provide some insight into which species observed on the site might be the most likely to collide with turbines. It is not meant to accurately predict which species will occur as fatalities nor is it meant to indicate potential impacts to birds associated with developing a wind energy facility at this site.

The small sample size of some species at the WREP, as noted by McIvor, indicates that use of the study area by these species is very low, which intuitively also means the risk to this species would also be very low. Moreover, the WREP site is a small tract of managed forest within a vast sea of coniferous forests, many of which are not managed for commercial timber harvest and likely provide better habitat for sensitive bird species. Therefore, no population impacts would be expected.

The Wildlife Society examined the issue of passerine fatalities at wind energy facilities and concluded that fatalities of passerines from turbine strikes generally are

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not significant at the population level (Arnett et al. 2007). Given the number of surveys conducted and the small numbers of olive-sided flycatchers, Vaux's swifts, western bluebirds and pileated woodpeckers recorded during the surveys, the data do not suggest the site is in an area where these species are concentrated. Therefore, no population impacts would be expected for these species.

In a similar vein, during a full year of avian baseline studies, only 2 golden eagles and 3 bald eagles were observed in the WREP project area. These numbers represent extremely low use of the area by eagles, some of the lowest of any projects in the western U.S. that I am aware of. Therefore, risk to these species is extremely low at WREP. Higher eagle use has been documented at many of the other wind projects in eastern Washington and Oregon despite the lack of any eagle fatalities at these projects.

Also, the National Academy of Sciences (NAS 2008) recently reviewed wind energy impacts on birds, and came to the following conclusion: "At the current level of wind-energy development (approximately 11,600 MW of installed capacity in the United States at the end of 2006, including the older California turbines), the committee sees no evidence that fatalities caused by wind turbines result in measurable demographic changes to bird populations in the United States, with the possible exception of raptor fatalities in the Altamont Pass area." The available information does not suggest population impacts to birds are likely.

To be clear, WEST's predictions of impacts to birds was based on avian use estimates (the number of birds) in comparison to wind energy facilities with similar avian use and post-construction fatality estimates. The collision risk model was not used to

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predict bird fatality rates. The DEIS was prepared by others who perhaps specialize in NEPA, and it is possible that they placed more emphasis on the risk index than is warranted given some of its limitations.

Q Do you agree with McIvor's testimony at 8:8-11 about WEST's failure to identify any bat species at the WREP other than the hoary bat?

A No. There are 14 species of bat likely to occur in the study area. Our acoustic bat surveys were able to classify bat calls to frequency groups that roughly correspond to groups of relative risk. Beyond that, the significance in the types of bats potentially present on-site must be correlated to information known about bats and wind turbines. Most of the bat activity we recorded at ground level involved high frequency bats. High frequency bat species are not typically associated with turbine fatalities. Based on data from 10 wind energy facilities in the Pacific Northwest, hoary bats and silver-haired bats have comprised the majority (93.4%) of fatalities. These species are considered low frequency species and they were more commonly recorded on Anabats elevated on met towers, but activity levels were not high compared to activity levels at sites with high mortality.

Q Continuing on the subject of bats, McIvor concludes that there is a "disproportionate impact [that] wind energy facilities are believed to have on forest bats." Is this true?

A No. While less may be presently known about these nocturnal mammals than diurnal birds, there is a considerable body of knowledge about these species. Wind energy facilities do not have a disproportionate impact on all "forest" bats. The species of bats impacted by wind turbines are primarily long-distance migratory tree bats (hoary,

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red and silver-haired bats). Even at that, these species appear to primarily be susceptible during fall migration. At forested sites in the eastern U.S., this has been the case: little to no mortality has been documented at these sites outside the fall migration period. It is expected that most bat mortality at WREP would be to long distance migratory tree bats (hoary and silver-haired) during the fall migration only, with little impact to resident bats in the area.

Q How, then, do you explain the graph prepared by McIvor at Exhibit 31.02, based on the three years of data sets for the WREP, that seems to suggest a vastly broad range of potential fatalities at this forested habitat site?

A Until recently, based on a limited number of studies (5), it was assumed that bat call rate data from Anabat units placed on the ground was roughly correlated with bat mortality, and could be used as a predictor of bat fatality. However, three recently-published studies have shown that bat activity data from ground-based detectors is apparently not strongly correlated with bat fatality, at least not in all cases. A recent study in Alberta found that bat activity levels determined from Anabat units raised on turbines were more closely related to bat fatality rates, and that there was no clear relationship between bat activity recorded at ground level and bat fatality rates (Baerwald and Barclay 2009). A similar study in Europe compared ground and raised Anabat detectors and concluded that assessing bat activity levels from ground level detectors only can be misleading, particularly when surveying high-flying species that are most likely to be at risk from wind energy development (Collins and Jones 2009). Finally, a study conducted in Illinois found that correlations between bat fatalities and bat activity levels from Anabat units placed on top of turbine nacelles was much more

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strongly correlated than data collected with Anabat units placed on the ground (Johnson et al. 2010).

Whistling Ridge completed three years of season-specific (June – October) bat surveys—more than previously offered by any other developer in the Pacific Northwest. The third year's data was collected from Anabat units elevated on met towers, in proximity to nacelle heights (also an unusual step for any Pacific Northwest developer). The elevated locations, and the associated surveys protocols were discussed in advance with WDFW.

The first two years of bat data gathered contained disparate results. For the 2008 study, the two Anabat locations showing the highest amount of bat activity were situated at near-ground levels. One of the high-activity locations collected data within a narrow forest corridor, with substantial tree growth along the corridor edges. The other collected data adjacent to a swamp, where significant insect activity and water source appears to have attracted either many bats or perhaps fewer, very active bats, whose activity was detected by the Anabat equipment. Neither of these two high detection locations represented or reflected the attributes of turbine corridors in a heavily managed forest. The new survey locations (Anabat placement) took into consideration vastly improved knowledge about bat behavior. As a result, WEST (and I believe WDFW as well) believes that the third year of data generated from the Anabats is far superior to the earlier years, is best correlated to known bat behaviors, and is the most reliable set of data. Therefore, although data collected in 2007 and 2008 provided important information on bat activity levels in the project area, the study conducted in 2009 when all four bat detectors were raised on met towers

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provide the best source of information for predicting bat impacts, as the met towers were placed in the same areas where turbines would be proposed.

With this in mind, I believe it is inappropriate to conduct the regression McIvor describes, using early Anabat data collected on the ground in less suitable locations, in order to try to predict fatality rates. Also, the differences in activity levels between 2008 and 2009 were based primarily on Anabat locations, not by year or elevation of the units.

Q Is there any correlation in scientific studies between wind turbines, bat mortality and the spread of insect-borne disease, as McIvor hypothesizes at page 12?

A No. Although some of the smaller bats such as little brown bat will consume mosquitoes, mosquitoes are not a major food source for bats. The primary prey of most bat species are beetles and moths, because it is more efficient to eat fewer large insects than it is to eat many smaller ones. Bats are generally not considered to be controllers of mosquitoes or other pests (see http://www.texasmosquito.org/Bats.html).

While it may be an interesting theory, one which I have seen postulated ala raptors' consumption of rodents and the predicted explosion in vector-borne illness if some raptors are eliminated by turbines, bats are not known to control mosquito populations. Population sizes of bats have nothing to do with the spread of West Nile virus associated with mosquitoes.

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Q Do you share McIvor's concern at 12:23-26 through 13:1-2 about the possible presence of Keen's Myotis and Townsend's Big-eared Bats (both State Candidate species and one a federal Species of concern) at the site?

A I concur that the WREP area is within the range of these two species and some potential habitat is present. However, we know from years of monitoring at wind projects across the country that neither of these two species has ever been documented as a turbine fatality at any wind project in the U.S. In addition, they are not long-distance migratory tree bats. Therefore, the potential to impact these species is very low at the site, as they do not seem susceptible to turbine collisions.

Q There is some vague reference in the McIvor testimony at page 13 about observations, in surveys of the WREP, of the Western Gray Squirrel at the site. Can you elaborate or clarify this?

A No. After reviewing this testimony, I went back and reviewed both our own records of the site surveys, and those of Turnstone Environmental Consultants, Inc. In WEST's records, there was one observation of a grey squirrel crossing a road in front of a technician. This was recorded as an incidental observation. There was not sufficient observation time to identify it as an eastern or a western grey squirrel. Our other site surveys revealed no squirrel observations whatsoever. WEST's surveys reported no western gray squirrels.

Turnstone Environmental performed elaborate, WDFW protocol-based western gray squirrel studies at the WREP site. I believe that Jeff Reams, a principal researcher with the company, is available to testify as a witness in this matter. While I cannot

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speak for him, I have reviewed the Turnstone baseline surveys, including the protocols and study parameters, which were developed in consultation with WDFW. The 26 survey polygons developed to search for western gray squirrel, as well as western gray squirrel nest surveys, disclosed no sightings of western gray squirrel at this site. The WDFW has reviewed these surveys and has expressed no concern about the possible presence of, or impact to, to the western gray squirrel.

Q McIvor raises similar issues to the cumulative impacts assessment at the WREP as Smallwood. Does your response differ from that identified above in regard to Smallwood's comments?

A No.

Q Do you have any comments regarding the mitigation measures suggested by McIvor at pages 15-16 of his testimony?

A I generally agree that his bulleted items 1-4, 6 and 7 are commensurate with the WDFW wind power guidelines or are appropriately and routinely applied in the state of Washington by siting agencies such as EFSEC. Insofar as bat-related conditions, I reference and incorporate here my comments above about the propriety of imposing specific, well-established BMP's as an adaptive management tool in appropriate circumstance as shown through monitoring results.

However, bullet 5 regarding eagle strike response is extremely inappropriate and poorly informed by science. The biology surrounding eagles is highly complex. There are different types of eagles that may occur on a wind project, and they are not

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necessarily reflective of each other in terms of their biological significance or function. For example, eagles that occur on a given site could variously include nesting resident pairs, young non-nesting juveniles, transitory birds, aged birds, sick birds, or wounded birds. The mere fact that an eagle fatality is found tells scientists nothing about whether the bird was ill, had eaten poison, was injured unrelated to the presence of the turbine, was a part of a breeding pair, simply passing through, or a juvenile, non-nesting bird. All of these factors, and more, must be evaluated before a response to the event can be made. Simply put, a fatality alone does not necessarily cause or imply an unacceptable impact to the eagle population. Shutting down of a turbine simply because an eagle carcass is discovered is an inappropriate, unmeasured and scientifically unwarranted reaction to the event itself. Adaptive management is the appropriate tool here: mitigation measures to be imposed regarding eagles should be based on an appropriate investigation into any incidents as they arise including assessment of the bird itself and inquiry into actual causation of injury or mortality; evaluation of the biology surrounding the particular species, using the best available science on the species and the roles that the species' constituent parts play; and should be designed to best effect an appropriate outcome to the type of bird and area population involved. This is a process that is best overseen and implemented by EFSEC as it has consistently and successfully done on all other wind projects on which it was the siting authority.

Insofar as McIvor suggests that BMPs to avoid strikes should be developed, recall that the numbers of eagles are so low at this site that they do not warrant individual mitigation measures such a strike-avoidance BMP, even if there were science supporting the efficacy of such a measure.

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